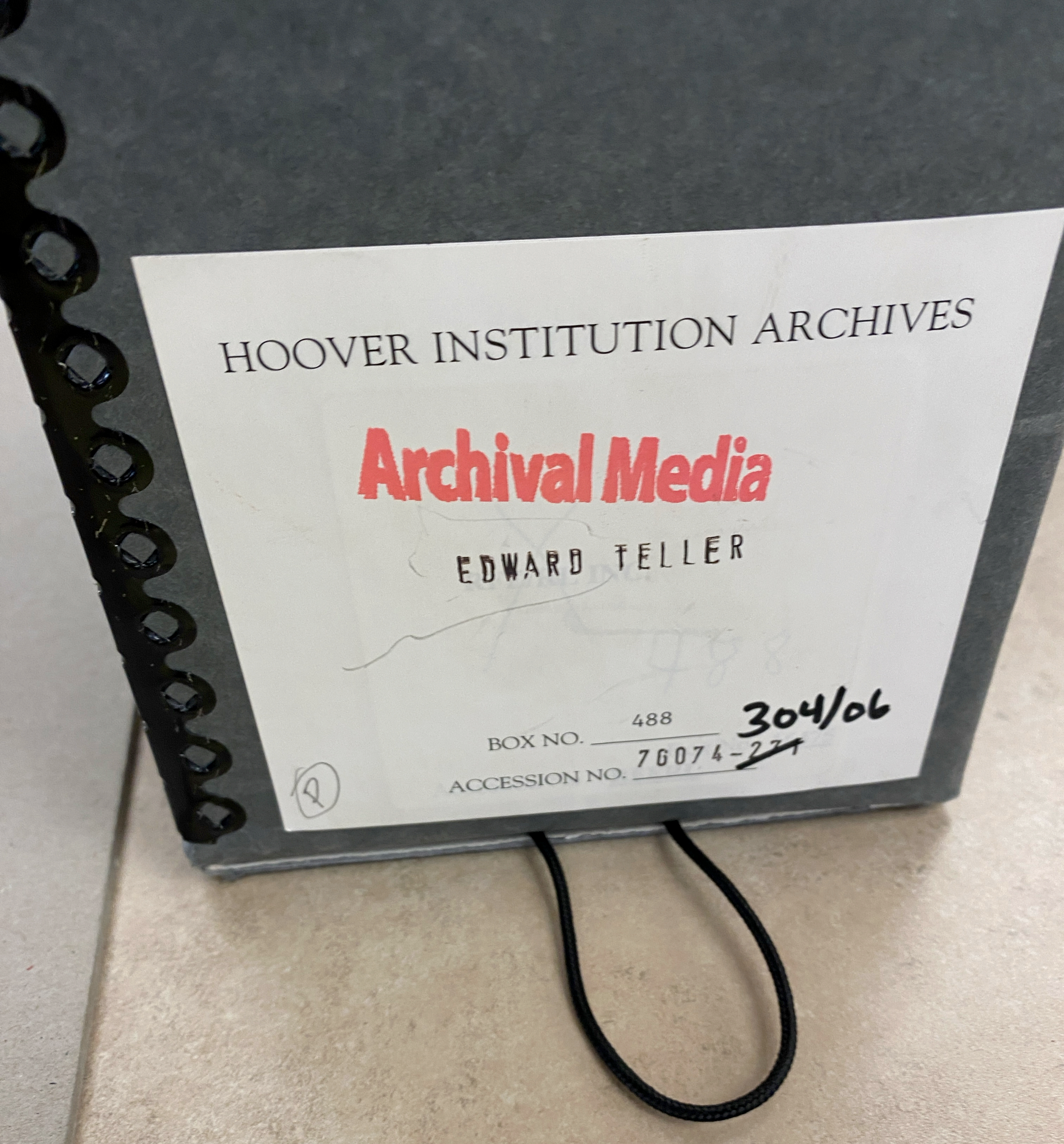

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1960-1966

EDWARD TELLER NASA

Stanford, California 94305-6003 USA

NOTE PAPER

Sept 19-27

June 2, 1966

Dr. John A. O'Keefe
Assistant Chief
Laboratory for Theoretical Studies
National Aeronautics & Space Administration
Goddard Space Flight Center
Greenbelt, Maryland 20771

Dear Dr. O'Keefe

Many thanks for your letter. I certainly am looking forward to seeing you on my visit to Maryland in September. However, it is impossible for me to state that I can see you on either of the two days you mention, that is, afternoon of Wednesday the 21st or the morning of the 26th. As the time approaches, I would suggest that you check my schedule with Professor Howard Laster, Chairman of the Department of Physics and Astronomy. He would be in a better position than I to tell you if I am free on either of the above-mentioned dates.

Sincerely yours,

Edward Teller

gp

Supt

NATIONAL AERONAUTICS AND SPACE ADMINISTRATION
GODDARD SPACE FLIGHT CENTER
GREENBELT, MARYLAND 20771

FILE. 23959

MAY 11 1966

AIR MAIL

Prof. Edward Teller
College of Engineering
University of California, Davis
Post Office Box 808
Livermore, California 94550

Dear Prof. Teller:

In answer to your letter of April 26 which arrived while I was on a trip: I am looking forward to a discussion with you on the occasion of your visit to the University of Maryland. Would you be willing to see me on the afternoon of Wednesday the 21st and perhaps also on the morning of Monday the 26th? It might be possible to explain things on the first of these days and then after you have had time to think about them to discuss objections on the second of these days.

Very briefly the answers to the two questions in your letter are:

(a) Meteoritic impact on sandstone produces inhomogeneous vesicular rock with numerous crystalline inclusions. (Crystalline inclusions are extremely rare in tektites).

(b) The probability that a chunk coming from the moon will impinge on the earth at the angle required to produce tektites by fusion stripping is of the order of 1,000th of the probability that such a chunk will hit the earth in some other manner. The parent body theory therefore implies that there have been a large number of impacts by materials coming at steep angles from the moon. The resulting craters would be ephemeral on the geological time scale. However, the Aouelloul crater in the Mauritanian Adrar may be one of them. It is surrounded by bits of glass which Campbell-Smith and Hey of the British Museum thought were tektites. I have seen it and I agree. Chao has seen it and does not agree.

I am grateful to you for your kind letter.

Sincerely,

John A. O'Keefe

John A. O'Keefe
Assistant Chief
Laboratory for Theoretical Studies

Sept 19th

April 26, 1966

Dr. John A. O'Keefe
3712 Thornapple Street
Chevy Chase, Maryland 20015

Dear Dr. O'Keefe:

Thank you for your most instructive letter of February 27th. Recently I had an interesting discussion with Harold Urey and now I am completely confused. At least in this respect it seems to me I have become like most people who ever looked at tektites.

I am deeply impressed by your detailed arguments concerning the appearance of constitution, the cavities, and the water content. I certainly want to know what happens to material like the sandstone which you consider when such material is splashed out under the influence of an exceedingly violent meteorite impact. I should believe that it will soon become possible to reproduce such impact by an experiment on a much reduced scale.

I would also like to know what is the probability of a chunk coming from the moon to impinge on the earth with the required glancing angle.

I doubt that I shall be able to absorb more of this material through correspondence. Next September, from the 19th through the 27th, I expect to be at the University of Maryland. Perhaps we might find occasion at that time to get together and I might learn more from you about this fascinating subject. In the meantime please accept my thanks.

Sincerely yours,

Edward Teller

ET:gp

astron.
Ph.D, Chicago 1941
g math + phy —
NASA — 1958. —

b. 1916
m 1941
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3712 Thornapple Street — same address
Chevy Chase, Maryland, 20015
February 27, 1966

Dear Professor Teller:

On your recent visit to Goddard you were so kind as to express an interest in the problem of the surface of the moon. Since it is generally agreed that meteorites impacting on the lunar surface have expelled portions of it with velocities sufficient to escape from the moon's gravitational field (papers by Shoemaker and Gault), and since a large fraction of the material so expelled will reach the earth (Opik, Gault, Chapman, Shute) it follows tha the problems of the nature of the lunar surface is an an intellectual one. The materials exist on the earth, and it is only a matter of recognizing them.

I have not been able to find out where, if anywhere, geologists list and describe specimens which they were unable to identify. It is difficult to arrange a dragnet operation, because the average geologist, confronted by a rock, has no idea what it is. It is necessary at least to prepare a thin section, and probably to have a chemical analysis, costing of the order of $100, to be reasonably sure. We are working on these problems; but for now we must probably study the rocks which the geologist have recognized as interlopers.

These fall into two classes:

a. Meteorites

b. Tektites and allied glasses. (Darwin Glass. Libyan Desert Glass, possibly Aouelloul Crater Glass).

Meteorites have been seen to fall -- if not the specimen in hand, then another like it. No one has ever seen any of the glasses fall; they last so long, compared to meteorites, that we are aware of them even though they are extremely rare.

Anything dislodged by meteorite impact from the moon might be expected to show two characteristics:

a. Petrographic marks of strong shock

b. Bits of meteorite imbedded.

The petrographic marks of strong shock have been identified by Shoemaker and especially by E.C. T. Chao, a marvellous petrographer in the Washington office of USGS. Chao identified, in 1960, the mineral coesite in the Meteor Crater throw-out. Coesite is chemically identical with quartz (SiO_2); but

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Dear Professor Teller:

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D SPACE
MINISTRATION

NASA HEADQUARTERS
1520 H STREET NORTHWEST
WASHINGTON 25, D. C.
TELEPHONE: EXECUTIVE 3-3260 TWX: WA 755

EPLY REFER TO ETL-268

August 10, 1960

r. Edward Teller
University of California,
Los Angeles Division
Los Angeles, California

Dear Dr. Teller:

Please send us two copies of the lecture you presented June 22, 1960 on the Peacetime uses of space. This lecture was ninth in a series sponsored by the University Extension at Royce Hall, UCLA.

Very truly yours,

James M. Watson
for Bertram A. Mulcahy
Assistant Director for Technical Information

NASA

1960-1966

it is, like diamond, a high-pressure polymorph unstable below 16 kilobars. It was previously unknown in nature; volcanoes produce only about 3 kilobars; it has been found since in at least one bomb test; it is not found in crustal rocks because the pressures are not high enough; and not in the mantle rocks (like diamond) because they don't contain free SiO_2. Coesite has been identified at several large craters since then, and is usually regarded as a clear indicator of impact. A higher polymorph, stishovite, was identifed and named by Chao. It was found at Meteor Crater, the Ries Kessel, and I think one other place.

In addition there is often a characteristically torn and rent structure; and fractured quartz grains. There is often a conversion of the original material to glass, sometimes in such a way tha the banding of the original feldspar crystal is still recognizable, though the feldspar is no longer birefringent.

At several meteorite craters (Ries, Wabar, Henbury) there have been found, imbedded in the impact glass, tiny spherules of nickel-iron, in proportions corresponding to meteorites. There is usually also schreibersite, an iron phosphide, which is practically unknown except in meteorites.

Among our list of materials suspected of lunar origin, we need not look for coesite or stishovite in most meteorites, because they do not contain free SiO_2. Diamonds are found in the Meteor Crater irons; also in ~~XXXXXX~~ ureilites (a type of meteorite); the latter often show the petrofabric of strong shock. The basaltic achondrites also often show evidence of strong shock. No meteorites, so far as I know, have been shown to contain the nickel-iron spherules. They do, however, undoubtedly encounter each other in space, whether they come from the moon or not, since their cosmogenic ages are some tens of millions of years -- this is enough in view of the amount of material in space.

Chao found nickel-iron spherules in tektites from the Phillipines and from Viet Nam. He identified schreibersite, also troilite (another meteoritic mineral, a sulfide of iron); and a long list of trace elements matching those in iron meteorites.

In 1965, Walter found coesite in a special and important variety of tektite, the Muong Nong type. He showed his data to Chao, and repeated it on another type of camera; there is no doubt about its reality. Coesite is probably fairly common in tektites of this type; it has not been found in the more ordinary tektites, such as the one I showed you, perhaps because the latter were formed as drops during the ablation of tektites of the Muong Nong type.

Another test of extraterrestrial origin concerns the cosmogenic isotopes such as Al^{26} and Be^{10} which are produced by primary cosmic rays. These are found in meteorites, but not in tektites. The allied glasses have not yet been XXXXX thoroughly examined. The absence of Al^{26} from tektites is generally regarded (and I think correctly) as excluding origin from anywhere beyond the earth-moon system. They have been in space less than 90,000 years, according to Anders; stone meteorites generally show ages of 1 to XX 60 million years in space, though one or two have recently been found with ages in space below 100,000 years. Even though the tektites had arrived as parts of a larger body (a reasonable hypothesis) they would probably have come from the outer parts. Anders calculates he would have got a positive resultdue to sampling unless the body had been more than 90 meters in diameter; in this case the body would have been more than big enough to furnish all the tektites of the great Australasian strewn field.

XXXFor these reasons, nearly all serious workers concede that tektites result from the impact of a meteoritic (or cometary) body on the earth or the moon. Urey and Barnes think is is the earth; Chao, Chapman, Walter and I think it is the moon.

The chemical composition of tektites is unlike that of any terrestrial igneous rock -- or at least any common one. Tektites have between 67 and 82% SiO_2 (petrologists still analyze into oxides rather than elements, except for trace elements). Terrestrial igneous rocks in this range belong in the granite family. Typically 80% or more of the total mass of such a rock can be expressed in terms of 3 compounds: silica (SiO_2); orthoclase ($KAlSi_3O_8$); and albite ($NaAlSi_3O_8$); these three XXX silicates being present in approximately equal amounts. There is a theoretical reason for this; it has been shown that this particular composition has a lower melting point than any other in the normal rock-forming range. Granites are either the last portion of a magma to congeal, or the first to melt. As compared with this standard, tektites lack K_2O and Na_2O. Many sedimentary rocks also lack K_2O and Na_2O because of leaching; hence it is generally agreed that if tektites are from the earth, they are from sedimentary rocks. Their composition would correspond to a sandstone, and in particular to a subgraywacke. Sandstones constitute about 12% of the land surface of the earth; subgraywackes are 1/3 of all sandstones, so the result is not a priori implausible.

At Goddard, during the last two years, some studies have been made which appear to us to show that tektites cannot be derived from sandstones. (We have not yet had a reply on these points.) In the first place, sandsones have about 1% water; while tektites have about 0.01% or less. The removal of water from sandy mixtures is one of the fundamental problems of the glassmaking industry, now in its fifth millenium; no quick ways have yet been found and there are theoretical reasons for thinking that they won't be. Because of their high viscosity, tektite glasses are orders of magnitude worse than industrial glasses from this standpoint, while the raw material, if terrestrial, is wetter. Hence one doubts whether impact can remove water without converting the glass into a sponge.

Second, the Muong Nong tektites sometimes contain angular voids. In a melted and congealed glass, the voids represent bubbles, and are either spherical or ellipsoidal. But in a partiallly melted mass, we would expect to see voids which represented the incompletely abolished spaces between the primary particles. These would be expected to show angles which might be oblique or even reentrant. Such things are sometimes seen; they are explained by Barnes as the result of incomplete melting.

This opens the way to a decisive test of the theory of sedimentary origin. A sedimentary rock consists of fragments of a crystalline rock (I should have said a *clastic* sedimentaary rock, to exclude limestones and coal). Each fragment is normally monomineralic: it is either a quartz grain or feldspar or mica or clay mineral. If the Muong Nong materials are imperfectly melted sedimentary rocks, then it would be expected that the fragments would retain their chemical identity, even though they might have become amorphous. Adler and I found, however, by microprobe analysis XX with a reaolution of about 3 microns, that outside of the SiO_2 nodules, the glass is homogeneous. The nodulesXXXXXXXXXX constitute less than 10% of the mass. In a sandstone of the composition of the specimenXs which we examined, sand grains should form 60% of the mass. These grains should have been about 100 microns across; note that the lower limit on grainx size is part of the essential property of sands, and accounts for their ability to let water through. We would have found sand grains or feldspar grains without trouble. We noted that where there were silica nodules, the boundaries were still sharp, as though the process of digestion had not begun. It follows that *these tektites were glass before the impact*. ThisXX conclusion is reinforced by the fact that these are the tektites which contain the coesite, which would have been destroyed by heating; note that the problem of dissolving the consituent grains is another famous problem in glassmaking; it also takes time. That it couls not be done in am impact is witnessed by terrestrial impact glasses which are invariably inhomogeneous. The famous Finnish geochemist Rankama went through 33,000X Australian tektites looking for inclusions without finding any except the nodules of SiO_2.

If the tektites were glass before the impact, they were igneous; and if igneous almost certainly not terrestrial. Studies by Ross Taylor have shown that in the impact process there is no large amount of volatilization; in particluar, not enough to convert a granite into a tektite.

Glassy fragmental materials are called by geologists tuffs; if welded , then welded tuffs. Thin sections of Muong Nong material do resemble welded tuffs.

Terrestrial welded tuffs are produced by XXXXXXXXXX *ash* flows. An ash flow is a natural process resembling an industrail gas-fluidized bed, in which small particles are involved in an upward-miving gas which separated them slightly from each other (or, in a dilute flow, very considerably) and results in a dense (or dilute) pseudo-fluid. Calculations by Adams and me show that this mechanism ought to work on the moon.

Ash flows are very flat -- flatter than basaltic lava flows. Lun 9, 60 cm. high, saw an unobstructed horizon in almost all directions. This is implausible for a basaltic flow . Radar data indicate similar flatness elsewhere.

Again, an ash flow compacts after it has reached its final emplacement. If it is not too thick, the XXXXXXXX compaction may reveal the underlying topography ("differential compaction") ("exhumation of the topography") just because the compaction is greatest where the flow is deepest. This would provide a handy explanation for the soft appearance of the lunar craters on the Ranger photographs. The smallest craters are not softened; perhaps they are post-flow. There is a detalied mathematical treatment of the cratering problem by Allen Marcus, a student of Neyman's; he concludes that you cannot balance the budget by cratering alone; that is, you cannot get enough debris out of some craters to bury the others. You need emission from the ground.

This, in short, is our theory.

Urey objects on 2 grounds;

First, the chemcial composition of tektites is too much like that of the earth's crust. It implies processes much like those on theearth, including the leaching of the XX original magma by iron which afterwards went to the core. The point seems to be well taken; and it is probably necessary to say that if the tektites came from the moon, then the moon came from the earth. There are real difficulties XXX with this idea; but XXXXXXXXX Huang and I think we see a way around them. I will tell you if you want.

Secknd, and much more important, Urey draws atention to the patchy distribution of tektites over the earth. Patchiness is unquestionably a real property of tektite distribution; e.g. 6066 none in England or Germany; but XXX lots in the Ivory Coast, Borneo and Cambodia. These patches cannot be produced by swarms of tektites coming from space, because such a swarm would be promptly disrupted by the sun's attraction unless the density were 10-6 or more; but in this cas a swarm covering Australia would pile tektites knee deep, literally. The actual density is roughly 20/km^2 To meet Urey's objection, it is necessary to suppose that each tektit e strewn-field is derived from a single chunk which made a grazing encounted with the earth's atmosphere, got into a decaying orbit, and fell apart scattering droplests and chunks (the Muong Nong material)under the perigee on several revolutions. This theory can be made to work (Miss Shute and me); there is apparently one observed case of a natural satellite coming into the Earth's atmosphere in about this manner (Chant, C.A., Jour. Roy. Astron. Soc. Canada, 7, 1913X, and later papers by Denning, Fisher, Pickering, Mebane, and me).However this theory clearly implies that the great majority of lunar debris reaches the earth in some other form. Conceivably the Aouelloul crater represents one chunk; possibly the scarcity of others is due to the fact that the surface layers are not weldedand therefore do not escape as solid chunks. XXXX Clearly, however, most of the lunar debris is getting XXXXX by unrecognized.

I have not mentioned Chapmen's beautiful work on the asutralites because I believe that they represent a tertial phenomenon (Muong Nong materials thrown from the moon, melted to form typical spalsh-form tektites, and finally re-entry into the earth(s atmosphere along a XXXX grazing orbit. But our calculations support his in saying that the observed degree of ablation rules out terrestrailXXXXXXX.origin.

The problem is a little complicated, but not really very difficult to pick up at this point. I should be glad to help guide you through to a start on it if you wanted to do something. The problem really needs more physics; the chemists have done what they could.

Sincerely yours,

John A. O'Keefe

John A. O'Keefe

Nasa

NATIONAL AERONAUTICS AND SPACE ADMINISTRATION
GODDARD SPACE FLIGHT CENTER
GREENBELT, MARYLAND 20771

file

Inter

FILE 6974
OCT 4 1966

AIR MAIL

Dr. Edward Teller
Department of Applied Science
University of California, Davis
P. O. Box 808
Livermore, California 94550

Dear Dr. Teller:

I have a few points which were left out in our discussion.

1. Fleischer, Price and Walker did take account of lead as well as uranium and thorium in their fission-track studies.

2. Al^{26} is formed by a (p,2pn) reaction from Si^{28} or from Al^{27} by (p,pn). This is according to Dr. Phil Cressy in our Geochemistry Laboratory. In a sufficiently large object it might also have formed from Mg^{26} by an (n,p) reaction but this is much less plausible than the other two.

3. The derivation of tektites by the ablation of a larger body in the manner which you and I discussed does not work very well if the large body is derived from a ground impact. The reasons are the following:

a. To penetrate the atmosphere with high velocity, it is required that a body have approximately as many grams per square centimeter as there are in the atmosphere. This implies a mass on the order of a thousand tons. (In fact, the largest meteorites observed to arrive whole (and hence at low speed) are on the order of one hundred tons while the smallest crater-forming meteorites are probably a factor of 10 larger.) The total mass of australites in Australia is estimated at a few thousand tons. A body of this kind, therefore, would give rise to an excessive concentration of tektites along the path on which the body approached the ground and a great deficiency elsewhere.

b. The trajectory of such a body would presumably be at a steep angle to the horizon since otherwise the atmospheric difficulties are multiplied by the secant of the angle from the zenith. But at a steep angle the ablation takes place at low altitudes in the atmosphere, particularly if the body is large. The resulting high pressures are likely to lead to the formation of very small drops or none at all. There will not be an opportunity for surface tension to shape the bodies.

c. In fact, Krinov found a large number of tiny droplets of iron near the impact point of the Sikhote Alin iron.

These objections do not apply if the body is in orbit around the earth. In this case we may expect that it will fragment and that the fragments will then pursue slightly different orbits all having the same perigee. This is sufficient to provide a wide distribution of the end products both along the path and in a region broadened in longitude. At the same time, the ablation at high altitudes will lead to large droplets.

Fragmentation followed by ablation and melting was observed in the case of the sustainer of the rocket which put Glenn into orbit. The work was done at General Dynamics/Astronautics by Genser et al.

Something similar to this seems to underlie the great Canadian fireball procession of February 9, 1913 for which I suggested the name Cyrillids. These bodies were observed along a line from Didsbury in Alberta, Canada to a point off Cape Sao Roque, Brazil. These bodies were very similar to what we now recognize as artificial satellites; that is to say, they traveled relatively slowly, were visible for minutes, all followed approximately the same path and appeared to be in level flight. C. A. Chant, a responsible Canadian astronomer, identified the phenomenon as the entry of a natural satellite of the earth in a splendid and neglected paper (Journ. Roy. Ast. Soc. Can., 7, 144, (1913)). The identification is fully supported by subsequent work in my opinion.

4. In addition to arguments for distribution, Urey regards the tektites as terrestrial because there is a detailed parallel between tektite chemistry and that of the earth's crust extending to all minor elements so far measured. The parallel is much closer, for example, than the parallel for meteorites. It follows, in my opinion, that if the tektites are from the moon, then we must go back to the ideas of Darwin about the derivation of the moon by fission of the earth. I am making some calculations on this at the present time.

5. The only way I know to get a numerical estimate of the amount of water which might have been in the magma before the ash flow is from a mathematical analysis of the fluidization processes. This was carried out by myself and Adams. It turns out that a lunar ash flow will resemble a terrestrial ash flow if g^2/e is the same, where g is the intensity of gravity and e is the flux of water out of the solid material in grams per gram per second. Since lunar ash flows are

apparently more extensive than terrestrial ash flows, it is possible that this parameter is somewhat smaller than on the earth. One might guess that the rate of an emission of water was less by a factor of 10 than that on the earth but not by a factor of 100. Since terrestrial igneous rocks have about 1% water, it would be surprising if the original magma had only 100 ppm of water, but with 1000 ppm the flow would behave in a way which seems reasonable. This must be understood as a very uncertain estimate.

6. If one had a reasonable length of time on the moon's surface, one might search for bubbles in ancient lavas. A trapped bubble has a shape which depends on the surface tension, the density of the fluid and the intensity of gravity. It turns out that surface tension is not strongly dependent on temperature. Thus it might be possible, using the ancient lavas which one would expect to find on the moon's surface, to measure the intensity of gravity in very ancient times. This, of course, would have some bearing on cosmogonic hypotheses.

7. Bevan French, in our place, has been a student of the processes of impact metamorphism. This field has developed during the past six years as a direct consequence of the program of lunar exploration. French has found indications of impact around the Sudbury structure from which the International Nickel Company draws about ½ gigabuck per year (most of the world's platinum and a good deal of its chromium and iron also come from Sudbury). It is not implied that the nickel comes from the meteorite but that the ore-forming process may have been triggered by the impact. It is evident that the moon will be the best place to study impact metamorphism and I believe that it may well revolutionize our ideas about a great many geological problems.

There is an entertaining story in this respect. Shortly after the beginning of the program on impact metamorphism, Shoemaker, fresh from his discovery of coesite visited the Ries Kessel, a famous circular feature in Southern Germany. He sent back some specimens of it to Chao who identified coesite in them by X-ray. Shoemaker reported this fact at an international congress of geologists a few months later, and pointed out that it meant that the Ries was produced by meteorite impact. A distinguished German professor arose at the meeting and said "For 55 years I have studied the Ries Kessel and no young man can pick up a specimen on a Sunday afternoon and tell me the nature of this event". Nevertheless, it is now generally admitted that Shoemaker was right and the distinguished professor was wrong and this point of view is largely accepted even in Germany. An important effect of lunar studies may well be to force us to look at our own earth with new eyes.

8. The best arguments which indicate the non-terrestrial origin of tektites are those which come from the glass-making processes. One of them can be given as follows:

a. Most authorities agree that if tektites are terrestrial in origin then they are derived from sedimentary or metamorphic rock. The two are essentially equivalent for the present purposes since metamorphic rocks are modified sedimentary rocks.

b. If the parent rock of the tektites was a sedimentary rock then it was an arenaceous, i.e., sandy rock as can be judged from books such as Pettijohn's "The Sedimentary Rocks". This connection is not absolute since there exist a few siliceous shales but the connection is generally accepted by workers in the field.

c. Sand grains are usually 100 microns or larger in size. A distribution in which the maximum size is less than 100 microns is quite rare (Pettijohn).

d. Some tektites have angular voids (Barnes). These have not been melted since they were assembled from separate particles. This interpretation of angular voids is the normal one and was brought forward by V. Barnes who espouses the terrestrial origin of tektites.

e. In the tektites which have angular voids, the material is found to be mostly a relatively homogeneous glass (O'Keefe and Adler).

f. The equation for the rounding of angular voids involves the viscosity of the material. The diffusion equation also involves the viscosity and it turns out that about 10^7 times longer is required to homogenize a 100 micron mass by diffusion than to convert a void of the kind observed in tektites from an angular shape to a spherical or elliptical shape. This ratio is almost independent of the time used or the diffusion coefficient.

g. This argument could theoretically be overthrown if stirring had occurred; it turns out, however, that the Reynolds number shows that turbulent motion is unimportant. In addition, there is direct proof of the absence of stirring from the form of the voids and from the presence of quartz grains which have been shattered but which are still together and in which the separate fragments have retained a common crystallographic orientation.

The same kind of result can be brought out by a direct calculation of the diffusion coefficient using the Einstein-Stokes equation. The temperature is limited in the case of some tektites, by the presence of quartz which would melt at 1700° and which would then solidify as an amorphous mass. It turns out that at this temperature the amount of diffusion required to homogenize the glass would require between 10^4 and 10^5 seconds. In this time a gob of liquid expelled from a crater would clearly have hit the ground. It would then have attached itself to particles of the ground like the glass in a fulgurite (lightning-made glass) but this is never observed in tektites.

9. In the long run, it is my hope that in ways which I cannot foresee, the study of the surface of the moon will illuminate the origin of the solar system and through it, the origin of the universe.

Sincerely yours,

John A. O'Keefe

John A. O'Keefe
Assistant Chief
Laboratory for Theoretical Studies

Enclosure

NASA

~~Govt~~
~~Agencies~~

NATIONAL AERONAUTICS AND SPACE ADMINISTRATION
WASHINGTON 25, D.C.

OFFICE OF THE ADMINISTRATOR

January 10, 1963

Dr. Edward Teller
Lawrence Radiation Laboratory
University of California
Berkeley 4, California

Dear Dr. Teller:

Mr. Webb has asked me to thank you for your letter of January 6th and to say that he would be very happy to see you at 11 a.m., January 22nd.

Sincerely yours,

Nina Scrivener

Nina Scrivener
Secretary to Mr. Webb

P.S. Our offices are located at 400 Maryland Avenue, S.W., Room 70067.

HOOVER INSTITUTION LIBRARY
Stanford, California 94305
NOTE PAPER

Jan 22

January 6, 1964

Mr. James E. Webb
NASA
Washington 25, D. C.

Dear Mr. Webb:

It was a very great pleasure to talk with you a few weeks ago.

I expect to be in Washongton on the 22nd of this month. Would it be possible for me to see you? One time that might be very good for me would be 11:00 a.m., if that is convenient for you.

Looking forward to hearing from you, I am

Sincerely yours,

Edward Teller

ET:gg

NOTE PAPER

NASA

November 23, 1960

Mr. Bertram A. Mulcahy
Assistant Director for
Technical Information
NASA Headquarters
1520 H Street, N. W.
Washington 25, D. C.

REF: ETL-268 and ETL-394

Dear Mr. Mulcahy:

We have been waiting for copies of Dr. Teller's lecture on June 22nd on the "Peacetime Uses of Space" in Royce Hall, UCLA and would be glad to send you copies, but we do not have them. I would like to suggest that you write to Mr. M. F. Thorne, who is handling this matter. His address if as follows:

Mr. M. F. Thorne
Assistant to the
Executive Vice President
Thompson Ramo Wooldridge, Inc.
8433 Fallbrook Avenue
Canoga Park
Los Angeles, California.

We am sorry for the delay in acknowledging your letter.

Sincerely yours,

(Miss) Genevieve Greteman
Secretary to Edward Teller

NOTE PAPER

NATIONAL
AERONAUTICS
AND SPACE
ADMINISTRATION

NASA HEADQUARTERS
1520 H STREET NORTHWEST
WASHINGTON 25, D. C.
TELEPHONE: EXECUTIVE 3-3260 TWX: WA 755

IN REPLY REFER TO ETL-394

October 4, 1960

Dr. Edward Teller
University of California
Los Angeles Division
Los Angeles, California

Reference: NASA ltr. dated 10 August 1960/ETL-268, sgd. J.M.Watson.

Dear Dr. Teller:

In the referenced letter we asked for two copies of the lecture you presented June 22, 1960, on the "Peacetime Uses of Space". This lecture was the ninth in a series sponsored by the University Extension at Royce Hall, UCLA.

Please advise us of the status of our request as we are anxious to receive copies of this lecture.

Very truly yours,

Edwin J Anderson/eb
for Bertram A. Mulcahy
Assistant Director for Technical Information

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1520 H STREET NORTHWEST
WASHINGTON 25, D. C.
TELEPHONE: EXECUTIVE 3-3260 TWX: WA 755

IN REPLY REFER TO ETL-268

August 10, 1960

Dr. Edward Teller
University of California,
Los Angeles Division
Los Angeles, California

Dear Dr. Teller:

Please send us two copies of the lecture you presented June 22, 1960 on the Peacetime uses of space. This lecture was ninth in a series sponsored by the University Extension at Royce Hall, UCLA.

Very truly yours,

James M. Watson
for Bertram A. Mulcahy
Assistant Director for Technical Information